Europe, Hungary and Yugoslavia as well) enjoyed an increase in per capita real consumption over the period 1973 through 1983 (*World Development Report 1986*)? And lest one object that GNP per capita only measures material welfare, infant mortality fell and life expectancy at birth increased in every European country over the six years from 1978 through 1984. Doubtless rates of economic progress are lower and levels of unemployment higher in the 1980s in Europe than was the case two decades earlier, but does this constitute a "continuing crisis"?

The authors and their research assistants have laboured mightily and ranged widely to produce these volumes, which contain many positive features. But in the reviewer's opinion, they have failed overall to inform sufficiently and challenge adequately the potential university-level student of history or economics. It is regrettable that these volumes were not limited to a shorter period, fewer countries and a smaller range of materials. This tighter focus would have permitted increased accuracy and relevance, as well as more detail to buttress often insightful generalizations.

Paul B. Huber
Dalhousie University

* * *

Bernard Waites — *A Class Society at War: England 1914-18*. Leamington Spa: Berg Publishers, 1987. Pp. 303.

Bernard Waites argues that in the decade from 1914 a conjunction of societal changes altered the form of the class structure but maintained the processes of social differentiation. The changes were the elimination of much primary poverty and the concomitant decline in the living standards of the skilled artisans, a redistribution of the national income in favour of the salariat and workers, a narrowing of wage differentials between and within the working class and middle class, a reduction of the large incomes derived from wealth and some redistribution of that wealth, an expansion of educational opportunities, and a strengthening of civic integration. Waites does not fall into the trap of assuming that the war caused these changes — the war, he says, concentrated them in a brief span of time and gave a demonstrable impetus to long-term cultural and demographic trends.

Using a culture-based model of class, Waites suggests that economically and culturally the working class in 1924 was more homogenous and more nationalistic than it had been in 1914. The pressures of economic mobilisation accentuated class divisions. Inflation, unequal income distribution and official restraints on labour created, we are told, a pent-up discontent which erupted in widespread industrial conflict in the aftermath of the war. Waites does not ignore but does pass over rather casually the fact that industrial conflict had been rife before 1914 and was scarcely muted during the war. It is a pity that he adheres so strictly to his self-imposed geographical limitations that he does not mention Clydeside. Where wartime distributions were notable and received much contemporary and, recently, some scholarly attention. Paradoxically, despite the political domination of the nationalist right, conscription and the direction of labour made for a more inclusive sense of national identity. A popular (he calls it demotic) nationalism was one legacy of the war. Although the war sensitised workers to class inequality (were they not already well sensitised?), Waites argues, nationalist sentiments in popular culture set limits on the political consequences of class feeling.

The war was "a great divide in the history of everyday working-class life" (122) which separated the poverty of Charles Booth's London from the relative well-being of the 1920s. And, Waites tells us, there was another divide apparent by 1923. London was in the favoured south. An imaginary line crossed England from the south of the Severn to Scarborough, dividing the economic stagnation of the north from the south's war-stimulated manufacturing centres. It is depressingly familiar today but, surely, was current long before the twentieth century. Despite the increased geographical mobility afforded by the railways, a north-south polarisation was as recognizable to nineteenth century politicians and writers as was Disraeli's two nations of the rich and the poor.

Unfortunately, Waites does not compare class structures on either side of the line and the distribution statistics which he uses to support his argument of significant income redistribution do not take geographical factors into account. An equally important limitation on his analysis is his adoption of the old sociological orthodoxy that a woman's position was determined by the male head of the household. Waites sets "aside any special consideration of women in the class structure and the war's impact on their condition and experiences" (6), even though the book'dust-cover shows a woman munitions worker.

There are few scholarly studies of the complex relationship between class and nation, the ambiguities and accommodations of which point to power relationships and social tensions. Waites' analysis is a welcome first attempt to deal with those issues during the Great War. The author perhaps sees the changes as greater than his evidence always suggests and there are some contradictions in the book. But, Waites recognises that he cannot come up with a definitive statement on class and industrialised warfare. This is a thoughtful and provocative work which raises more questions than it answers.

Gerald Jordan
York University

* * *

W.L. Warren — *The Governance of Norman and Angevin England 1086-1272*. Stanford: Stanford University Press, 1987. Pp. xv, 237.

The first criterion for review of any monograph ought to be whether or not the study is a significant addition to the already existing literature. By that standard this volume is redundant. Chapters one and two recapitulate data to be found in any standard text of medieval England; indeed, Chapter two repeats in small the already published first volume of the series — The Governance of England — of which Warren's is the second volume. Chapter three, four and five are adapted from Warren's masterful *Henry II* (1973). The other chapters, numbers six and seven, cover the period 1189-1272, about which there are surely writings in abundance — even, perhaps, already too many.

The subliminal claims about The Governance in England Series, i.e., that there have been no comparable studies for the last two generations and that this series synthesizes recent research on English medieval government, not only defy logic but are palpable untrue. What is true is that although Warren lists all the studies which have been published in the twentieth century, he makes little or no use of them in his treatment of Anglo-Norman governance. Is it enough to cite an author, R.H.C. Davies, for instance, in *King Stephen* (1967), and not acknowledge that the argument one is making about the reign of Stephen is substantially that of the cited authority? Warren implies that his argument — that Stephen, as king, was deliberately planning a decentralized government — is his own; it is, in fact essentially Davis' argument.

There is another instance of this practice in Warren's study. He had early access to Marjorie Chibnall, *Anglo-Norman England 1066-1166* (1986). Chibnall argues, with considerable elegance, for the "gradual assimilation of Norman and English institutions over several decades" (2). Warren, citing her study because it reviews recent work (24, *Bibliographical Note)*, ignores all that she has written on the subject of the relationship of Norman and English institutions in order to argue that the Normans did not adapt Anglo-Saxon government. Given the absolute paucity of evidence — which Warren admits often — he ignores Chibnall at some peril.

One justification for yet another study on governance might be that the author had such felicity of language that he or she provides a new clarity or undestanding of abstruse arguments. In this instance no such justification exists. One example of tortuous language must suffice: "There were, of course, some laws defined by the crown of generally universal application embodied in 'law codes,'